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ON THE VARIATION OF STRENGTH DURING MOVEMENT

J. H. O. Reijs

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The significance of measurements of variations in strength during movement with respect to remedial exercises, design of artificial limbs, etc., is first discussed. The author describes a dynamometer for measuring strengths of various muscles in terms of a weighted pendulum, and problems associated with statistical variations due to physiological conditions such as fatigue. ~~A paper of Franke's is then criticized, and the author's own definition of absolute strength defended. Recklinghausen's results are examined and found to agree well with the author's.~~ Measurements of hand movements are then described and compared with those of Herz with which they are in good agreement. Likewise the author's measurements for ad- and abduction of the leg agree well with those of Bethe and Franke. Extensive tests of hand grip and strength of the back muscles are then given for 2,000 people of both sexes, and ages from 6 to 60 years and are found to agree well with those of Quételet and Dementjeff. The relation of the strength of the left hand to lefthandedness is examined for both men and women.

Author

In an earlier article on the absolute strength of the muscles in the human body[1] I drew up the following definition:

The absolute strength of muscles in the body under the influence of the will is the greatest strength that can be exerted in the course of contraction. I employed this latter definition because the strength with which a movement is made, undergoes continuous changes in the course of this movement which, being dependent on three factors: Schwann's law, mechanical relationships and the extent of innervation, are very complicated.

Now, however, we are engaged in seeking the solution to this problem, from two aspects, the theoretical and the experimental. The first way was pursued

[1]Nederlandschr. Tydschr. V. Geneesk., 1912 and Pflügers Arch. f. d. ges. Physiol. 160, 1915.

/Numbers in the margin indicate pagination in the original foreign text.

by von Recklinghausen[2], the second by Herz[3] who has, however, published few results. Recently this problem has received attention from several investigators such as Bethe[4] and Schlesinger[5] who conducted investigations of the muscle stumps treated according to Vanghetti and Sauerbach's method; Bethe and Franke[6], who worked on both healthy and amputated cases using a dynamometer, the author[7] and Franke[8] who also used these methods of measurement in order to arrive at the absolute muscle strength. /235

It is not merely interesting from a theoretical standpoint to learn about these curves for all or at least many movements of the body, but it also has great use in practice. The results of these measurements could perhaps be used in the "Physiology of work"[9] in order to construct the tools of work as economically as possible in terms of the Taylor system, while in addition the experiments are very welcome as lending a solid basis for the theoretical calculations.

My aim, however, was still another and, indeed, the following:

The ideal resistance movement in remedial gymnastics would provide a

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[2]H. v. Recklinghausen. Mechanics of the limbs and artificial limbs (Gliedermechanik und Lähmungsprothesen) Berlin, 1920.

[3]M. Herz. Text for remedial exercises (Lehrbuch der Heilgymnastik) Berlin-Wien, 1903.

[4]A. Bethe. Contribution to the problem of voluntary movement of artificial limbs (Beiträge zum Problem der willkürlich bewegten Prothesen) Münch. med. Wochenschr., 1917.

[5]Schlesinger. Technical utilisation of kinoplastic arm stumps (Technische Ausnützung der kinoplastischen Armstümpfe) Dtsch. med. Wochenschr, 1920.

[6]Bethe and Franke. Contribution to the problem of voluntary movement of artificial arms (Beiträge zum Problem der willkürlich beweglichen Armprothesen) Münch. med. Wochenschr., 1919.

[7]J. H. O. Reijs. On the variation of strength during movement (Over de verandering der kracht tydens de Beweging) Neder. Tijdschr. V. Geneesk., 1920.

[8]Fr. Franke. The strength curve of human muscles with voluntary innervation (Die Kraftkurve menschlicher Muskeln bei willkürlicher Innervation) Pflügers Arch. i. d. ges. Physiol., 1920.

[9]Physiological Organization of work, 1917. The human motor, 1914 (Organisation physiol. du Travail, 1917. - Le Moteur Humain, 1914) J. Amar.

resistance which increases during the movement in the same degree as the strength of the patient also increases during this movement, or at least we want to make the muscle work during the entire duration of the contraction and continuously at the maximum.

Ling was the first to state this requirement. For the resistance he took the strength of the remedial gymnast who is, during an exercise, in a position to provide this resistance in the way desired. Zander constructed his resistance apparatus according to this principle[1]. Since he took levers with sliding weights as the resistance, so that the force varied sinusoidally, it is then rather obvious that even if while such moments at which the strength of the patient and that offered by the apparatus are respectively maximum or minumum, correspond for the two, yet otherwise the two forces vary in very different ways. Only to a superficial inspection is there matching in this case. For this reason I did not agree with Lagrange[2] when he asserted that with Zander's apparatus there is, "at all stages of the movement perfect proportionality between the force to be overcome and the force deployed by the muscle".

In this Zander merely consulted his own feelings when he sought for the time during the movement at which the maximum or the minimum strength was in effect. His equipment was not much better than that of Krukenberg wherein the opposing force remains unchanged during the movement. Herz has now undertaken, as was mentioned, to establish experimentally the variation of muscular strength exerted in all simple movements, but has published only 3 diagrams since he, as he informed me in a private communication, believed that there /236

[1]Levertin, medico-mechanical exercises (Gymnastique médico-mécanique) Zander, Stockholm.

[2]Methodical movements and mechanotherapy (Les Mouvements méthodiques et la Méchano-Thérapie) Paris, 1909, pp. 26-27.

was not much interest in the work. Bethe and Franke published curves of various arm movements.

In order to flesh out my definition of absolute power, I have repeated these measurements of varying strength during different movements and have had a dynamometer constructed which has a certain resemblance to that of Herz. This dynamometer consists of (Fig. 1) a stand A, on which wheel B is mounted, the latter having 60 holes spaced 6° apart. A fork, C, which can be moved around the axis of the wheel can now be fixed at each of the holes by means of a pin and can be locked by the nut, D. A handle, E, can be screwed fast to this fork at various distances from the middle point. Under wheel B sits a much smaller wheel F and on it a pendulum, G, with a sliding weight, H, and a pointer, I, moves over a graduated arc. A string, L, is now wound around

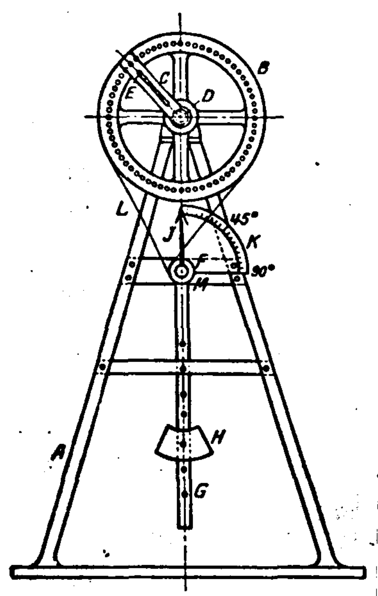

Fig. 1

wheels B and F and attached to the latter by a pin, M, so that F always turns in the same sense as B, so that the pointer always moves over the graduated arc. The force due to the pendulum G then increases as the sine of the deviation. Because of the large difference in the circumferences of B and F (respectively 1615 and 135 mm, that is 12:1) F will turn through a large angle when B turns through a small angle and the opposing force rapidly increases. The person to be tested is now so positioned that the axis of the joint to be investigated, e.g., the elbow-joint, accurately forms an extension of the axis of B. Then the position of the part remaining at rest, in this case the upper arm, is read off with regard to the wheel B. Then the handle is so mounted that the person can easily clasp it. Thereupon the fork C is brought into the position from which the force is to be investigated and this position is recorded as also is that of the handle on the fork. While the upper arm is now held firmly and the feet and the back have also received the necessary support, the person being tested bends his arm, and immediately the pointer, I, strikes out and the pendulum exerts a strong force by means of a scarcely perceptible turning in the elbow-joint. Special care should be taken that the person carries out the movement slowly and uniformly.

The position of the pointer is recorded and this value is reduced to the number of kilograms according to the calibration, which were operating on the handle E, or what is simpler and equally accurate, the sine of the deflection angle will itself be taken instead of the angle. For in all these tests I was much more interested in relative values than in absolute strengths.

This value also gives a measure of the force with which a particular /237 movement can happen. Since we are now studying this force from a number of aspects, we shall provide a presentation of the behavior, of the variation of the force during this motion.

How shall we now go about these measurements?

In the first place it proved that the same person on different days with the same position of the weight, registered different values of the fork and handle positions. This is related to the state of mind, the influence of which was very noteworthy, and with greater or less state of well being, tiredness, etc. Thus, I proceeded to take all the phases of a movement to be tested one after another and some days later investigated the movement in reverse order of phases in order thereby to exclude tiredness which might have appeared in the later stages. This happened with some persons (4 to 10). If less than 10 persons were investigated, these were selected from a greater number and were those which showed the most uniform results in 10 succeeding measurements of a specific movement, which I selected to be the bending of the elbow-joint.

The reason that I initially undertook measurements on ten persons, and repeated the tests for these ten times, which themselves consisted of a measurement of 10 to 14 positions, and the reason that I later however, limited all these numbers, lies in my experience, contrary to that of Herz, that although there were no permanent harmful consequences, yet certainly temporary, rather disagreeable effects revealed themselves in the persons being tested. On one day when we were investigating the supination of the hand, one of the women being tested for example, menstruated some days early and with unaccustomed violence. And even if this is not a harmful result, it still shows what an effect these tests can have.

Also headaches, dizzy spells in elderly persons and painful muscles frequently appeared.

Let us now come to my results and I want to begin with the plantar flexion of the foot which was the starting point of the whole investigation. I studied this movement with four persons, for both the right and the left foot, and for four positions in each case. The tests were carried out twice as we said, in order to exclude the effect of fatigue (which furthermore was not great).

The result of the measurement is as follows (Test 121 to 134):

Angle of the Joint	Stroke of the pendulum	Sin x • 100	Force in kg
114° (plantar flexion)	36°	59	384
102°	46°	72	463
90° (normal position)	60°	87	560
78° (dorsal flexion)	68°	93	598

The force thus increases with the extension of the calf muscle as is to be expected in such a simple mechanical relationship as we have here. The 78° position should be noted as the strongest dorsal flexion. /238

Since in my first tests of the strength of the calf muscles, the bend in the foot joint was 90° and in my definition of the absolute strength, I treated this as the greatest strength which arises during a movement. Then another 6/87 must be added to the previously given figure of 5.25 kg per square centimeter of physiological cross section and thus we arrive at about 5.6 kg per square centimeter of physiological cross section.

I must take advantage of this opportunity to mention a paper by Fr. Franke[1] which recently appeared, pertinent to absolute muscle strength. It given the following definition.

"Absolute muscle strength is the maximum strength (tension) per square centimeter of physiological cross section plane, which the muscle is in a condition to exert with maximum innervation and favorable length."

I must here remark that a difference still remains - which does not arise from this definition - between the absolute muscle strength of a muscle as a whole, which is the subject of my definition, and the per square centimeter

[1]Pflügers Arch. f. d. ges. Physiol. 184, 1920.

of physiological cross section.

In all these definitions maximum innervation must be taken as a matter of course so that this no longer needs to appear in the wording of the definition.

However, there exists in this definition a tautology. The, "favorable length", is indeed the length at which the muscle exerts its, "maximum strength".

I have brought this out while I was talking about the, "greatest strength", which can be exerted in the course of the, "contraction". Thus, I must then also refer back to Franke's remark: "it would have been better if I had stipulated a particular muscle length". Really I have indeed done this: that is, taken that length at which the strength is greatest (the most favorable length) which Franke also did. Hence, I can not perceive the improvement which the definition of Franke hoped to contribute and believe that all that is contained in his definition also exists in mine.

I would also then be blamed because I took as a foundation the values for strength obtained with the test volunteers and the measurements of anatomical preparations.

This, however, I also hold to be false. I have indeed made use of anatomical preparations in order to be able to form a judgment on the calf muscles and especially on the succession of phases, the surfaces and the circumference. However, I have then calculated the volumes of the calf muscles of the people tested according to castings in plaster of Paris.

And did not Franke act in the same way? He too measures the circumference and the cross sections of preparations and then the circumferences of the arms of the people under test.

In addition, he also has to measure the lever-length and application point, etc., on preparations although he possesses a control-method in x-ray photography, a control which in my opinion should only be employed with the greatest care.

Since Weber [1], the position on the toes has always been taken as the movement for which absolute strength was calculated, because fairly simple anatomical and mechanical relations then persist.

Franke, however, chose bending and stretching of the elbows. Although /239 it is desirable to become acquainted with the absolute strength of other muscle complexes, and however noteworthy the efforts of Franke may be, it appears to me that there is here a much greater possibility of inaccuracy. This fear does not become less if we read that the thickness of the soft parts was determined only, "approximately" (p. 317), and that: "the very complicated relationships between the two muscles had to be greatly simplified" (p. 314) and that, "the size of the lever-arm found in this way for the biceps and brachialis did not greatly deviate from that which O. Fischer obtained from his calculations", etc.

The attachment of the muscles must be brought to a single point: the advantage which the different muscles bring into motion must be calculated, etc.

The construction of the triceps further appears so developed that calculation of the physiological cross section was disregarded. On this account Franke made his calculations only with the anatomical section. This number, therefore, came to only a small value.

I must also remark that the lacertus fibrosus appears to have been forgotten.

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[1] Dictionary of Physiology (Handwörterbuch der Physiologie), 1846.

In the Table which Franke gives of the values of absolute muscle strength found by different workers, stand some numbers which I would also mention but about which I would state how and in what direction they should be improved. From this it occurs to me that for the same muscle groups, the values of Henke (5.56 kg), Knorz (5.9 kg) Hermann (6.24 kg, somewhat too big) and those of mine (5.6 kg) are sufficiently close to each other.

We now consider the numbers obtained by Franke, in more detail. Here follows his table.

Table VI

	Be	Di	Fr	General Average	Average Bend
Triceps	16.8	17.9	19.8		
Biceps	11.4	12.4	8.9[1]	13.5	11.1 (force in kg per sq. cm.)
Brachialis	12.1	12.5	9.7		

It must then astonish us that with the test subjects Fr., the stretching force per square centimeter is greater than with other people while their bending strength is smaller; that their stretching strength is more than twice as great as their bending strength while with other people this ratio is much smaller; that deviations of about 50% of the mean appear in a total of 9 numbers; that all the numbers are much greater than were previously found, which mostly are obtained with leg muscles. These results appear to me not likely to inspire great confidence in the methods used, especially when we take into account the arguments of Recklinghausen (loc. cit.). The latter believes and even grounds his further investigations on this: that various constants, including K, the absolute muscle strength, "are one and the same for all human skeletal

[1]This number was given in the summary: 8.2.

muscles" (p. 18) an opinion which I can not at the moment accept.

Henke and Knorz [1] found for the flexor of the arm the value of 8.991 kg for the right and 7.38 kg for the left.

I cite these figures because here, naturally, an experimental method is used.

In my earlier article in this journal[2] I posed the questions: Does /240 there exist an essential difference between the muscle on the right and on the left?

Is there a special difference between the different muscles of the human body; in particular are the leg muscles stronger than the more delicate arm muscles: Questions which have not in the meantime been solved.

Von Recklinghausen also gave a figure for the absolute muscle strength, namely, 3.6 kg. We now wish to investigate to what extent this figure may be brought into agreement with my figure of 5.5 kg. We shall then see that there is substantial agreement.

Von Recklinghausen stated, as his definition of the absolute strength, that force which the muscle can exert when it has its natural length; while I took the greatest strength to be that which the muscle can exert during contraction. We both considered the strongest degree of innervation. We are now investigating how great this strength is when we consider his Fig. 18a and his Table III. It then appears that, at the natural length, a force of 12.1 kg can be exerted, and that, however, a force of 17.8 kg can also be exerted (Test 4). This is perhaps not the very greatest force which the muscle can develop. On dividing this number by the cross section (Q = 3.36 sq cm) we

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[1] Zeitschr. rat. Med. 1865 and 1868 Dissertation, Marburg, 1865.
[2] Vol. 160, 1915.

then obtain the absolute strength in accordance with my definition as 5.3 kg and this approaches my value of 5.6 kg very closely.

If we now pursue the matter in the opposite direction, that is, now attempt to reduce this figure obtained by me to that at the natural muscle length, while we make use of von Recklinghausen's tables, we again encounter the difficulty that the extended length of the gastrocnemius and soleus occur at different angles, namely, at 80° for the soleus and at 70° for the gastrocnemius (Table XXI). If we take the angle under tension for the Achilles tendon with the stretched knee as 70° (para 14h) and calculate the absolute strength for that, then I arrive at a figure of 4.05 kg which also again is not very far from that of Von Recklinghausen: 3.6 kg.

Von Recklinghausen's calculations are too clever and too logically carried out for me to make many comments thereon. However, it occurs to me that, especially in the calculations of Q, there is too much leeway for inaccuracies for me to take his figures, without more ado, to be as accurate as my own.

There remains only the question of discussing which is the right definition of absolute strength, that is, at which length of muscle we will obtain the, "absolute strength": the length at which the greatest, the "absolute strength" can be exerted, or another length, for example, the "natural length" as von Recklinghausen puts it.

In this connection it might be mentioned that the natural length should be considered from various factors, some of which must be measured on the cadaver; and that for various muscles with tendons difficult to gain access to, the extended length is very difficult to calculate; that the extended length of various muscles cooperating in a movement is different (for example, gastrocnemius and soleus) and that, in conclusion, the natural length, that /241 is the length of the muscle which is not shortened by innervation, has little to do with the absolute strength which is the strength at the greatest possible innervation. On all these grounds it seems to me that my definition, which comes immediately from experiments for determining this magnitude, is more reasonable.

One becomes aware of this by comparing the figures for the absolute strength, that there is no agreement with respect to the definition. Attempts have been made, as I did earlier, to reconcile the numbers.

Von Recklinghausen's observations are indeed worth more attention. For this reason I have also commenced to verify several of his experiments and hope shortly to return to this matter.

One matter, however, I should like to point out now since I have already published an earlier investigation hereon, namely, on the stretch-curve of noninnervated and weakly innervated muscles, or, as von Recklinghausen designates it, the length-tension relationship of noninnervated muscles and of muscle innervated to less than maximum.

My method, as a method for investigating the tonus and the influences which make themselves felt thereon, is a repetition of the experiments of Mosso and Benedicenti [1]. For that reason I also call my apparatus the myotonometer. While, however, Mosso experimented with the foot, I chose on various grounds the ring finger of the right hand and while immobilizing the hand and the bottom-joint by means of the apparatus and the end-joint by means of a finger-ring with a pin, I obtained exclusively a movement in the joint between the bottom and the middle phalanx and thus a stretch of the M. flexor

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[1] Arch. ital. de Biol., 1896.

digitorum sublimis, the very same muscle on which von Recklinghausen made his tests.

In order that the outward-stretching force should always work perpendicularly on the finger, I placed the band on which the weight is attached about a wheel on whose middle point the moving joint rests. Fig. 2 makes this clear.

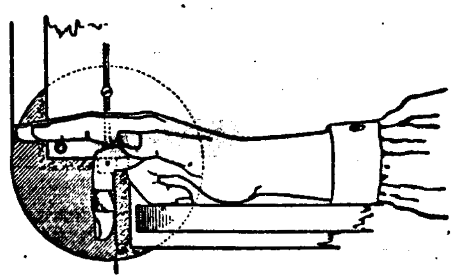

Fig. 2

The outward-stretching is derived from the inflow of water which can again flow out through an opening in the vessel. This vessel is nevertheless balanced out by means of a counterweight so that at the beginning of the experiment no weight is operating on the finger.

If I now sketch the stretching-curve, then I shall obtain a line as shown in Fig. 3. This curve agrees quite closely with that which Mosso ob- /242
tained with the calf muscles, while the objection which von Recklinghausen raised against Mosso's method (unlacing of the foot, the fact that the sandals did not remain fixed) is not valid in my method.

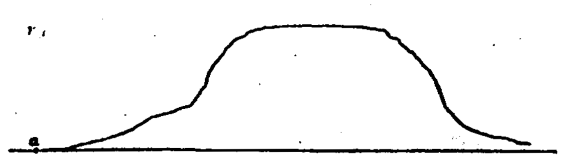

Fig. 3

The similarity of these results speaks well for their correctness.

I published this method as early as 1917 [1], and in 1920 made a report of the results which were obtained with this myotonomotor[2].

While the finger was left quite slack in my experiments, one should here speak of extension curves or, length-tension curves of the noninnervated muscle.

If we now compare these curves with those of von Recklinghausen, we then find two great differences:

1. Von Recklinghausen's curves consist of three parts forming angles with each other.

2. These angles are not open above. There is thus formed a broken, concave line opening upwards while mine is convex upwards.

Von Recklinghausen, who is acquainted with Mosso's investigations as well as those of Langelaan [3] who also obtained gently curving lines without corners, attempted to explain this difference by pointing to the irregular structure of the calf muscles, whereby the various fibers come to their natural lengths at different points in time [4]. This explanation is however not valid for my curves which are obtained on the same muscle as that on which he experimented. For the explanation of this difference however, the following points should be noted:

We commence with the rest length of the muscles; von Recklinghausen calculates his curve from the natural length on, and this rest length is a little

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[1] Handelingen XVI Ned. Nat en Geneeskundig Congres 1917.
[2] Nederlandsch. Tijdschr. v. Geneesk. 1920; 2e. Helft. No. 26.
[3] Arch. f. Physiologie 1901.
[4] Also for this reason I did not regard the determination of the absolute strength at the natural length as valid.

larger since there is a stretch due to the opposing muscle. Our curves show only a small lengthening of the muscle.

In my experiments only one joint of the four over which the said muscle passes, is moved and then only over a part of its range of movement. Now I load the muscle still more than was ever necessary in my experiments on the /243 tonus, then it turns out that the length becomes greater only slowly and that indeed there is then agreement with the curves of von Recklinghausen.

Thus in our curves, the e" part of von Recklinghausen does not appear and in regard to the remark about the rest length, perhaps e also does not appear, so that we now have to deal only with the e' part which he ascribes to the tonus of the muscle (para. 22g) with which I am in agreement.

Von Recklinghausen works statistically; we proceed dynamically by virtue of the inflow of the water and it is well known how great an influence is derived from the method by which the muscle comes to a particular length.

Finally it should be remarked that the natural length is not a constant, and the tonus has the property of wanting to maintain a certain length (the plastic tonus (Sherrington) or tonus-level as I called it).

From this it is evident that the first part follows a rising course - the part of increasing tension. There is, however, a limit: the tonus yields, the muscle lengthens and we see a more or less steep part of the curve whereas at the same time the tonus test is ended. This part is that of increasing length.

If now the load is not excessive, then the line will run very nearly horizontally, when there is an "allongement subséquent" (subsequent lengthening); if the weight is decreased, then the muscle shortens only very slowly and in the end faster, but in addition it only rarely returns to its original length.

There is a "residual lengthening" which von Recklinghausen does not wish to acknowledge, but which I met with in the great majority of my experiments. Only in a few experiments in which I first let the muscle lie for some time in static contraction, (2 minutes with 1 kg) did I see a restoration even further than to the original length (where it should immediately be stopped). Therefore, this non-restoration cannot be ascribed to experimental errors.

Here also, in the length-tension curve of the noninnervated muscle, I believe my presentation - "la forme de casque" (helmet-shaped) as Benedicenti designated it - should be treated as the right one; however, this is only a small part of von Recklinghausen's curves; in the end, the difference is not too great. However, as long as tonus plays a part in the extension-curve, we do not arrive at a straight line. I hope shortly to be able to report in more detail on these experiments too.

Now, another few words on the muscle which is innervated less than the maximum.

In that case one commences with the assumption which until now was valid almost in general, that with an innervation strength of 1/2, 1/4, etc., the whole muscle comes gradually into contraction. The experiments of Keith-Lucas, and Kraft and Eisenberger [1] have revealed that with local excitation, and with a continuous increase of the excitation, the muscle reacts with a discontinuous increase of its action, and that the various muscle fibers thus have different excitation thresholds.

Then a small excitation will thus not bring the whole muscle into contraction, but only a part and then the muscle, under a submaximal excitation,

[1] See, for example, Amer. Jour. of Physiol., 49, 1919.

would not always reach its smallest length, but a length between this and the natural length, depending on the strength of the excitation. /244

We must now return to the change in the force with which a movement occurs, during the course of the movement; whereupon we shall also have the opportunity to go into the article by Bethe and Franke.

A comparison between my figures and curves obtained completely experimentally and the purely theoretical arguments on the angular moment curves of von Recklinghausen would go beyond the compass of this work and I have refrained completely on this account.

These figures will, however, be useful for testing the theoretical considerations.

The results of these measurements may be graphically represented in two ways, namely on a circular arc, on which the angles designate the positions of the joint, or in an orthogonal coordinate system. For better understanding of the lines, I give here only one figure according to the first method, in which dorsal and palmar flexion of the hand are plotted together. To make the matter clearer the hand is also sketched in (Fig. 4 and Table A).

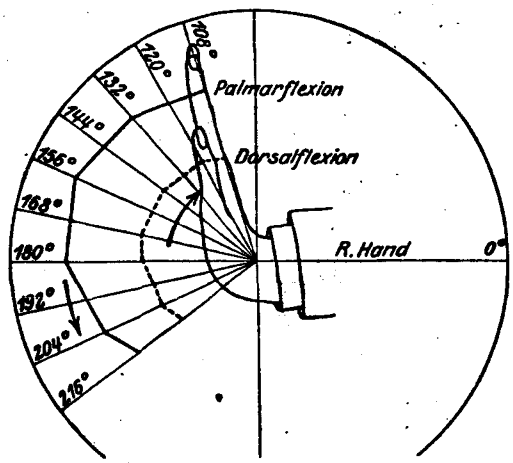

Fig. 4

Table A

Angle of the Joint Degrees		Sign of the Angle of Deflection of the Pendulum: Palmar-flexion	Dorsal-flexion
108	Dorsal-flexion	36	21.5
120		37	23
132		38.5	23
144		39	23.5
156		40.5	23.5
168		39	24.5
180	Normal-position	39	23.5
192	Plantar-flexion	36	22
204		36	22
216		31	20

The palmar flexion was investigated with 10 persons (tests 111 to 120) each of whom carried out the motion 3 times, each time in 10 positions, and the dorsal flexion with 4 selected persons (tests 141 to 144). The uniformity of the lines is noteworthy in the first place. Apart from the furthest bend (216°) the greatest difference in palmar flexion is only about 10% of the greatest value; and in dorsal flexion, about 12% while the smaller values /245 are regularly arranged on either side of the maximum. It takes the form of a fairly pure ellipse.

The second noteworthy feature is the uniformity of both lines, the dorsal flexion being like a reduced reproduction of the palmar flexion. Both have their maximum at the same position (156° and 168°) and this is indeed the position of the hand which occurs most frequently. This uniformity, in my opinion, indicates the great anatomical correspondence on either side of the joint. These facts strike us as more remarkable if we have also seen other curves in which quite other relationships arise.

In this figure too, the ratio of the average force of the two movements is evident, namely 10:6. This is the same ratio that Herz (loc. cit.) gave.

I must strongly emphasize that the values in this graphical representation are not to be compared with those in later such presentations for we must instead make very large drawings for the abduction of the leg.

We now pass on to the movements of which Herz gives complete diagrams, both in polar and in Cartesian form. The latter however, appear to me to be the most suitable for comparison with my values.

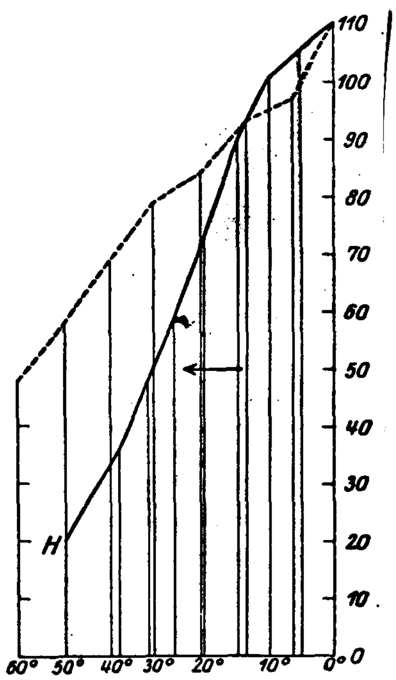

Fig. 5. Abduction of the leg.

Herz gives ab- and abduction of the leg and pronation of the hand.

We shall first consider abduction of the leg (Fig. 5). My measurement itself comprised 18 series, with both legs of 3 persons investigated, in 8 positions, each with a spacing of 8^{o}, and thus over a range of 56^{o}. Herz measured in 9 positions over 48^{o}. He does not mention from how many series his values form a sample, nor with how many people he carried out the measurements. It would not be amiss to take the values of the left and right leg separately. A priori it is to be expected that both sides would contribute the same results. Further on I wish to show, for the ab- and adduction of the arm, that this may also be proved experimentally. /246

So that the lines obtained by Herz can now be compared with mine, I have accurately copied Herz's figures for each of three movements and sketched in my lines so that both lines coincide at the starting point. Since the absolute numbers do not here enter into the matter, there can of course be no objection to this. The continuous line marked H is that of Herz, the dotted line is mine.

We then see that the character of both lines is the same, a symmetrical, only occasionally irregular, descending line. Here also it appears to be Schwann's law which governs the line, which does not surprise us in view of the relatively simple mechanical relationships. The difference between the two lines is this; mine falls much more slowly than that of Herz. From what cause, I do not know. Could it be due to the fact that I excluded fatigue better? Or does it arise because Herz knew better how to immobilize and thus was better able to localize the movement? Since Herz gives so few hints about details, it can not be said with certainty.

As a control, four years after the first test I have again measured this movement, now with 10 persons, each time in 7 positions, each series twice (series 151 and 152). The results were, however, the same with very little deviation.

Fig. 6 represents the abduction of the leg. The continuous line is again that of Herz, the dotted line mine, the results of measurements on 7 persons,

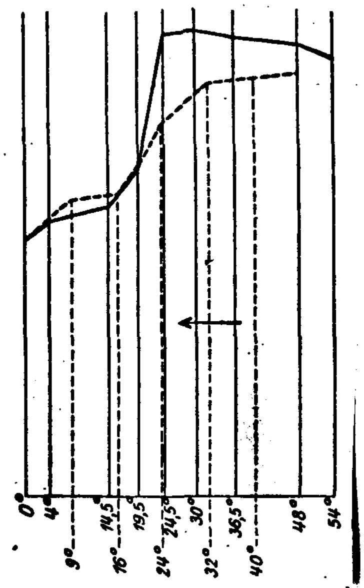

Fig. 6. Abduction of the leg.

each time in two series, and again sketched in such that the lines coincide at the zero position. These lines also show a satisfactory agreement which might have been even greater if Herz had also given a figure for the position at 8°. The later difference might again be the result of insufficient immobilization, now, however, in the case of Herz.

Now we have the third diagram, pronation of the hand (Fig. 7). Here a question presents itself. According to the figure, Herz measured these values over a range of 189°. According to R. Fick, however, this range amounts to 185° to 190° on the ligament preparation, 150° to 160° on the muscle preparation, while Strasser [1] estimates it at 120° to 150° in living /247
persons and thus on average, 135°. It was not possible for me to investigate this movement over more than 120°. At the further positions fixation on exerting strength was too painful.

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[1] Textbook of muscle and joint mechanics. (Lehrb. der Muskel- und Gelenkmechanik) Vol. IV, Berlin, 1913.

The difficulty now is that when I sketch my line on Herz's figure, I do not know at what point of the movement I should let both lines coincide. I have therefore chosen the 184° position, but might just as well have let the 5° position or the average points coincide. If one remembers this, then in this figure too the agreement can be said to be satisfactory throughtout.

Bethe and Franke also gave strength curves for pro- and supination which they have measured over a range of 200°, thus even greater than Herz. Is it possible that they have investigated this motion with the elbow not bent at right angles, so that in addition outer and inner rotation in the shoulder joint could take place?

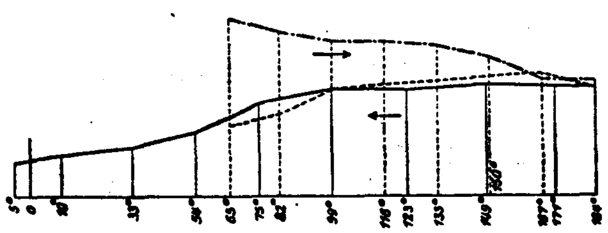

Fig. 7

In no other way can I explain these greater values. These curves then, are again not appropriate for a comparison with my curves.

My line is the result of measurement on eight persons, each time in two directions. The 184° position represents for me the outermost supine position, and thus with the surface of the hand upward, and of course with the underarm horizontal and the elbow joint bent at right angles. If we treat supination also in combination with pronation, then that will be the uppermost line in Fig. 7. Herz gives no data about it. These lines of pro- and supination are drawn to the same scale so that it reveals that the supination strength is greater. This line was the result of measurements on 9 persons. The variation of the force is in both cases very similar and since I have seen about the same in the dorsal and palmar flexion of the hand, which I have presented in Fig. 4, then the question arises, as to whether this is always the case with opposing muscles. The answer to this question must be in the negative. And as an example I cite the ab- and adduction of the arm. In order to show at once how completely the right and left agree, I have also superimposed /248 these on another figure (Fig. 8). These are the results for the same 9 persons. Each movement was carried out twice in opposite directions. Here alone we have, in fact, the result of 648 measurements.

It is seen how closely parallel the lines move from left to right; and further, how the right arm, apart from a small exception, always has the advantage, and how much greater this advantage is with the higher values of abduction. As the force in one end-position (the 0° position is that where the arm lies against the body) approaches that in the other, it is seen how, in addition, there is now a great difference between the two lines, one concave downwards the other concave upwards. These lines, the measurement of which stretched over about 14 days form, in my opinion, an overwhelming proof for the correctness of the experimental arrangement, etc. The continuous lines in the figure represent the average of left and right. If we now compare these figures with the curves which Bethe and Franke give for this movement (Fig. 3 of their article on the line designated by S) then we find a striking resemblance for the abduction.

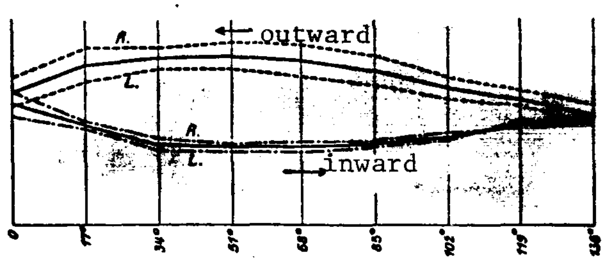

Fig. 8

Only in the left part of the curve are Bethe and Franke's curves greater than mine. Unfortunately one cannot detect such a similarity with respect to the abduction. My curve is for the greater part lower, but between 102° and 136° it is higher. Now Bethe and Franke accept at once, with respect to this line, that it appears to form an exception, since in it, unlike in their other curves, a maximum of the curve does not agree with a minimum of the antagonist curve. I note in this connection, that this rule holds true for my curves for ab- and adduction.

I cannot agree with the explanation they give for those of their lines which deviate. For they say: "The true minimum of the motion curve in the negative direction (abduction) would lie at the left end and the curve would fall steeply here up as far as the zero position, unless the body hindered a further approach to itself".

I must inquire whether they concluded that the curve would drop to zero /249 when the curve rises straight from right to left and only in the last part shows a trivial fall. The body is such a physiological resistance that the muscles are syntonized thereon, thus affecting the length and stretching force. What then should be considered the cause of the deviant type - deviant with respect to the other curves and in comparison with my curves - I cannot understand.

I must here direct attention to another point where my findings are not in harmony with those of Bethe and Franke: As I have already stated, my curves for right and left arm are very similar, as also follows from Fig. 8. On considering the similar anatomical structure of left and right, in my opinion, nothing different should be expected. Bethe and Franke however find, "in almost all movements a weaker and also genuinely different curve for the left extremity" and as an example, point to their Fig. 2 in which the curves for forward and backward motion of the upper arm are given. Indeed, there we encounter some curves which are striking because of their irregularity; which are partly higher for the left than they are for the corresponding curve for the right. I would indeed expect such an irregularity to enter if I had produced curves from a small number of experiments; this disappears however as soon as we superimpose a satisfactory number of experiments. I cannot imagine a drop such as this figure shows at 30°, both for R+ and for the L- curve in considering the uniform function. If the drops are restored around small values (2 kg for the R+ curve and 4 kg for the L- curve) then the irregularity disappears and it seems to me that such an occurrence will arise if more figures are superposed in arriving at the average.

In this way I would also explain the depression at 60° in the curve for the two women on bending their elbow (Fig. 5). It is, however, difficult to accept that, in the case of the women, at one moment during bending a depression of the force would enter at the one time while the curves for men reveal only a regularly rising behavior.

The bending and stretching of the arm indicate values which in their mutual relationship have a greater similarity to the bending and stretching of the hand. I shall give values here only in tabular form for the inner rotation of the arm in which a self-consistent change shows up with what are quite different anatomical relationships.

I have studied the inner rotation of the arm with the upper arm horizontal and raised sideways and the under arm bent at right angles. The 0° position is in agreement with the under arm placed vertically at that height whereas the upper arm is turned as far as possible to the outside. /250

Position of the Joint (stretched)	Armbend	Stretch	Inner Rotation
0°	29	23	25
17°	33	28	26
34°	37	29	28.5
51°	42	33	28.5
68°	45	33	30
85°	45	34	30
102°	44	37	29
119°	36	33	24
136°	28	26	23

Bethe and Franke also give curves for bending and stretching of the arm. For comparison I have superimposed in Fig. 9 the average curves for four men in Bethe and Franke (B+ and B-) and my curves. It is seen from this, much to my joy, that this is confirmed and that there is almost complete agreement between the bending curves of Bethe and Franke and mine, an agreement which might have been absolute if the authors had also given figures at 70° and 100°. The agreement is extensive also for stretching but not as great as for bending.

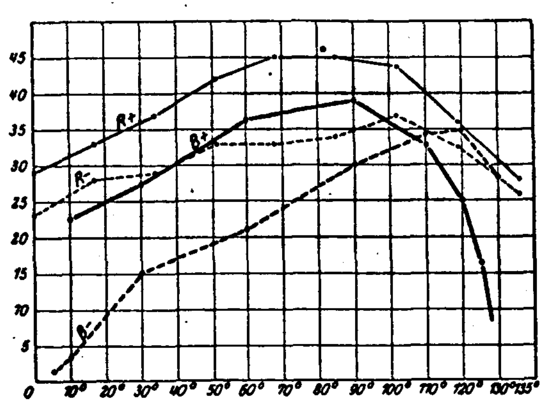

Fig. 9

Finally I also investigated the force in an entirely different way, namely that of lifting the trunk from a bent over position, the so-called loin strength. For this purpose I fastened a Collin's dynamometer to two rods, one being put under the feed while pulling on the other. By inserting a chain it was possible to enlarge the distance from the hands to the ground by degrees. I obtained the following figures:

Distance of the hands from the floor in centimeters	Average force in kilograms
20	118
27	123
40	114
45	114.5
54	126
60	131.5
70	139

Thus one obtains a smaller value if the trunk is about horizontal. Since the height of the person being tested plays an important part in this test, I should really have studied this with persons of equal height. These figures give the number of kilograms which the dynamometer indicated.

What immediately strikes the attention in all of these curves and tables is the uniformity of the change. Each curve has a relatively simple form. A curve may show a smaller fall (e.g., in pronation) than another (e.g., abduction of the leg), but there does not occur more than one bend in the curve. Nor do we find: rise, fall, rise, fall. There are either fairly straight lines or bent lines with a certain curvature which can be convex upwards or downwards. /251

I believe that in view of the anatomical structure, this uniformity is, indeed, also to be expected from theoretical considerations. It should be expected that the greatest force always lies at the point of movement where the muscles work most. If we accept this, then in these curves we should look for the location of this point and also where it should lie in tools and artificial limbs. One can also easily calculate from these curves the construction of gymnastic resistance apparatus in order that the greatest possible resistance can be offered to the movement of the muscle at each instant. I do not believe that more general conclusions can be drawn from these experiments and curves.

Again I must point out here that the figures for different movements should not be compared with each other. Since I changed the attachment of the handle and the weight and used a more heavily constructed dynamometer, the figures are not directly comparable. Finally, in order to be able to make calculations, I will give one of Herz's tables on the average strength of the movements here described. I might well have changed the figures for this purpose, but I wished to use Herz's diagram for comparison; he has not even calculated his diagram with average forces. Although I am not in entire agreement with these figures of Herz, (thus I find the adduction of the leg stronger than the abduction) I shall not comment on them here. In these figures the stretching force of the knee is taken as 100.

Hand palmar flexion	10
Hand dorsal flexion	6
Pronation	15
Supination	15
Underarm bend	45.3
Underarm stretch	36.7
Abduction arm	96
Adduction arm	136
Plantar flexion foot	130
Abduction leg	119
Adduction leg	112
Trunk bent backwards	122

In connection with these dynamometer tests I have investigated the grip of the hands and the lifting power of the back muscles with 2,000 persons: 1,112 male and 888 female from 6 to 60 years.

Any person who presented himself was tested. Students from various /252 schools, members of various organizations, teachers and office workers, nurses, patients as well as members of a church congregation. I sought without any selection whatever to collect a rich material in the age range from 6 to 60 years.

Each had to squeeze with his left and right (hand) and then pull on a rod on which the dynamometer was mounted and which by means of a cross bar was held solid under the feet while a cross bar fixed above was pulled upwards as powerfully and uniformly as possible. The trunk was then bent forward. Each test was allowed to be repeated and the greatest figure obtained was recorded. I used three Collin's dynamometers which were newly calibrated always after a certain time.

Just as Quételet insisted, at least 10 measurements were combined for each age; the irregularity of curves so obtained clearly shows that this number is too small and that the total number of my measurements is also too small to be able to obtain smooth lines. One should, however, recall that Erisman in a similar undertaking with workers in central Russia still did not obtain regularly running lines with a total number of 103,175 persons, and that he combined the results for each two years*, and also combined the results for each five years from 30 to 40 years and above that for each ten years [1].

My figures are however decidedly too small to be able to take account of the scattering of the results or the effect of increase in age.

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*Translator's note: Probably "each two years up to 30 years" which has been omitted.

[1] Investigation of the corporal development of factory workers in central Russia.(Untersuchung über die körperliche Entwicklung der Fabrikarbeiter in Zentralrueeland) Tübingen, 1889.

My intention was twofold. In the first place I wished to set up an average value which could serve for comparison. For this reason I gave the results in Table B in kilograms shown on the dynamometer.

It is evident how typical oscillations enter into this, which can be smoothed out at will, by adding or dividing of two or more neighboring numbers.

In the first column is the age, in the second, third and fourth columns are respectively the right and left pressures and the pulling force of men - then the same values for women.

My second intention was of a more general nature, namely to establish certain rules about the relationship between the strength of the right to the left hand, of the man to the woman and on the trend of the strength curve from 6 to 60 years. For the last named curve I combined the three figures obtained (right and left hand and lifting strength) in order to avoid irregularities in the lines as far as possible. In this way I also established an index. /253
However, it is still necessary, if one is to obtain regular lines, to take some years together (Fig. 10).

Finally I compared my results with those of Quételet[1] on the lifting strength of the back muscles and with those of Dementjeff. The last named reference appeared in Russian and I must here refer to Erisman's paper (loc. cit.).

We shall only treat the strength relationship for left and right. Even while I was setting up the tests, which stretched over 1 1/2 years, it struck me how great a number of people are strong on the left rather than on the right, a number which surpassed the percentage of lefthandedness which, as I thought, ran at about 1.5 to 4%. If one considers the results it is evident

[1]See: Boigey, Precepts and Maxims of Physical Education (Préceptes et Maximes d'Education physique) 109, Paris, 1920.

Table B

Age	Male R	Male L	Male Lift	Female R	Female L	Female Lift	Age	Male R	Male L	Male Lift	Female R	Female L	Female Lift
6	7	6	32	6	6	32	34	45	43	157	26	25	94
7	8	8	34	8	7½	33	35	43	41½	153	29	26	87
8	10	9½	44	9½	9	39	36	44	40	151	24	23	87
9	11	11	50	10	9	44	37	44	41	150	22½	21	86
10	11	11	65	12	10	46	38	42	39	137	23	23	84
11	12	12	64	12	11	52	39	39	35	150	24	23	76
12	14	14	69	17	14	51	40	44	40½	144	27	23	92
13	19	18	78½	16	14	60	41	40	38	145	25	24	82
14	22	19	86	18	18	79	42	45	42	154	25	22	80
15	30	28	110	22	21	80	43	44	41½	153	22½	22	75
16	34	33	129	23	21½	84	44	43	39	142	23	23	92
17	34	31	114	23	23	81	45	47	42	144	19	19	66
18	39	35	135	23	21	80	46	44	40	144	21	21	83
19	40	36	140	24	23	94	47	47	43	140	23	21	70
20	40	37	142	24	23	94	48	48	44	144	21	23	71
21	44	41	146	20	19	87	49	44	42	145	24	25	81
22	46	42	156	27	26	100	50	36½	33	134	17	18	72
23	46	40	150	25	23	96	51	35	32	129	19	19	80
24	44	42	153	24	21	88	52	37	37	131	20	19	84
25	43	42	153	25	25	96	53	42	37	133	23	22	93
26	45	42	163	26	24	92	54	36	33	121	22	22	73
27	44	41	157	30	29	111	55	45	39	118	17½	17½	81
28	45	44	154	27	25	101	56	37	33	117	20	20	70
29	41	40	143	24	24	93	57	37	31	127	19	19	70½
30	46	40	151	29	28	90	58	30	26	100	19	20	57
31	42	41	153	24	22	84	59	31	29	85	18	17	63
32	45	41	150	25	22	83	60	39	34	131	19	19	74
33	43	41	157	25	23	86							

that a very great number are stronger on the left than the right.

Among the 1,112 males, 278, that is 25%, and among the 888 women, 273, /254 that is as much as 30.7%, were stronger on the left than the right. In addition there were another 91 men and 115 women who were equally strong on the left and the right.

The question is now whether we may, without further ado, speak here about lefthandedness, and the answer in my opinion is in the negative, especially if righthandedness is defined as a preponderance in length, girth, strength, blood pressure (Hecht and Lungstein), capability, etc., of the right arm. In my opinion not every factor is relevant in and by itself, with the possible exception of capability, to righthandedness. How great the strength advantage may be in determining right- and lefthandedness, I do not venture here to settle. I intend to determine this in a more detailed investigation. I might very well remark here that my results agree with those of Lombroso, that in women, lefthandedness occurs more often than in men, contrary to those results of Ogle, who has just established more lefthandedness among men (5.7% against 2.8%).

Next to these small numbers we should, however, put a number arrived at by Biervliet, which approaches mine, namely 22%[1].

Gaupp remarked about this number that it has hitherto remained unexplained. Would the explanation perhaps lie in the way in which lefthandedness is established, that is, whether one uses the assertion of the person himself or whether one measured his strength? It also appears that no agreement exists between the figures for "skeletal lefthandedness" and "functional lefthandedness". From the studies of Hasse and Dehner with 5,141 soldiers, it appeared that according to their statements only 1% were left handed while, however, in 7%, the left arm was longer and in 18% the armlengths were equal. When I measured brute force which agrees quite well with the physiological cross section of the muscles, it is not impossible that for this reason, I obtained a much greater number of people stronger on the left, yet without being in a position to call them lefthanders. Perhaps in the long run it would be desirable to make a better definition of the concept of right- and left-handedness.

In order to be able to form a judgment on the strength of lefthanders, I studied the hand grip of 100 lefthanded men and 100 lefthanded women. The result of this study was as follows. They were all people who stated that

[1]Compare, however, these data: Gaupp, On the righthandedness of human beings (Gaupp, Über die Rechtshändigkeit des Menschen) Jena 1909, from which I have borrowed.

they were lefthanded.

36 men and 24 women were stronger on the right /255

51 men and 66 women were stronger on the left

13 men and 10 women were equally strong on both sides.

Lefthandedness should thus definitely not be defined as implying a stronger left hand; this is stronger only in one-half of the lefthanded men and 2/3 of the women.

To what extent then is one hand stronger in lefthanders? The total for men with strong right hand is 1,198 on the right and 1,068 on the left, thus an imbalance of 10.85% of the stronger hand. For the right-strong women, this number is 16.8%. Of the left-strong men these numbers are respectively 1,735 and 1,987, thus 12.7% and for the women 1,227 and 1,489, thus 17.6%.

With the women, the imbalance in strength of a hand is thus somewhat greater than with the men, they are also distinguished by this fact.

I should point out here how much the left hand exceeded the right in strength in a number of patients of an insane asylum, which I investigated. The small values which I then found turned out noteworthy in another connection.

How much in general is the right or the left hand stronger? The total number of kilograms of the right-strong people (including as well the equally-strong) is: for men right 30,919; left 26,888; right thus 4,031; that is 13% more with the strongest hand.

And for women: right 5,173; left 6,092; left more by 919 or 15%.

Finally I investigated how the left-stronger men were distributed by age. The result is seen in the following table.

6 - 15 Years,	73 out of	230	= 31.7%
16 - 25 Years,	62 out of	292	= 21.2%
26 - 35 Years,	52 out of	189	= 27.5%
36 - 45 Years,	51 out of	215	= 23.7%
46 - 60 Years,	40 out of	186	= 21.5%
	278 out of	1,112	= 25%

Thus a fairly uniform distribution.

The ratio of the strength of grip in men to women is, according to these numbers, 5:3.

We now consider the variation of strength with age. As mentioned, for this purpose I combined the numbers, for each age, of the right and left hand with that of the back strength. Since in this calculation it is desired to /256 obtain a flowing line, in the case of the men I combined each two years from 17 years on, and the results for each 4 years from 27 on, and again for each two years from 51 years on. In the case of the women, I combined the results for each two years from 16 years on; each 4 years from 24 years on. The lines in Fig. 10 thus have arisen and only a slight irregularity appears (in

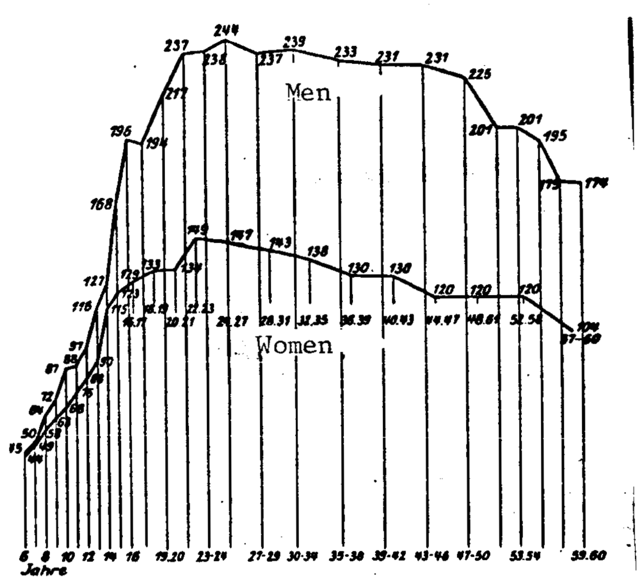

Fig. 10

men from 17 to 18 years). We see a sharply rising line which in men reaches its maximum at 25 to 26 years, and thereafter a very slowly falling course so that the strength between 21 and 43 years decreases very slightly while only from 43 to 60 years a much quicker fall enters.

In the case of women, the line climbs somewhat slower, the maximum being reached somewhat earlier and the fall being somewhat faster. These lines reveal a very high degree of agreement with those which give the growth and development of different body measurements (length, weight, etc.) as published by various workers.

From these curves we can make a judgment on the strength of men and women by taking account of the surface area. We find for this the ratio of 5:3, the number generally found for this ratio.

Finally I compared my results with the data of Quételet[1] and Dementjiff (see also Erisman, loc. cit.). These give the pull of the back muscle in kilograms. /257

In Fig. 11 I have plotted these three lines, the continuous line being for my results, the dotted line for Quetelet's, and the -·-·-· line for those of Dementjeff.

The agreement can be said to be surprisingly good, especially if one considers the big difference in the time, national character, and material in the various investigations. Despite all the circumstances, growth, development and decline are not only similar, but by and large take the same course.

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[1]See: Boigey, Precepts and Maxims of Physical Education (Préceptes et Maximes d'Education physique) 109, Paris, 1920.

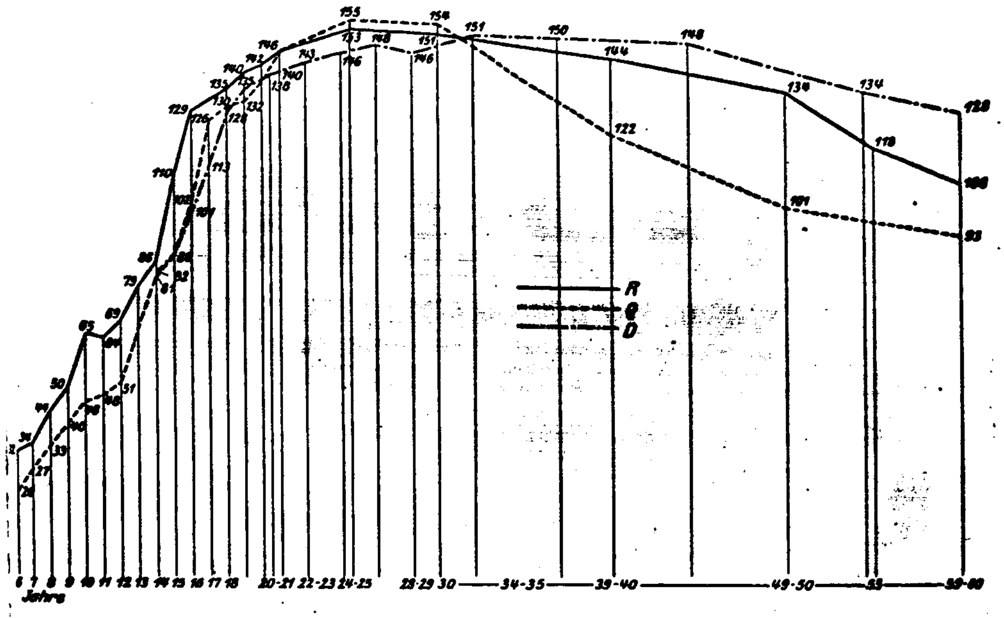

Fig. 11

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